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ABSTRACT

Wage rates and earnings can mislead public and private decisionmakers about the social benefits of certain kinds of education and training investments. This confusion arises because (1) workers and employers prefer employment contracts that downplay productivity differences among workers doing the same job; and (2) important dimensions of educational and training accomplishment (skill, knowledge, and competencies) are often not signaled to potential employers and therefore have limited influence on job allocations. This situation results in significant productivity differentials among workers receiving the same pay for the same job. Some differentials are related to inefficiently signaled educational and training accomplishments (or academic achievement). This paper develops a simple signaling/implicit contract model of the labor market. True productivity depends on general intellectual achievement and educational credentials, but since the former is unobservable, pay is based on credentials and supervisory assessments of doubtful reliability. The labor market tends to overcompensate credentials and undercompensate academic achievement. Next, the paper refutes an assumed equality between simple wage and individual marginal revenue products. A hypothesis equating productivity with wage effects of general intellectual achievement is also refuted. Finally, the paper develops a method of estimating academic achievement's true impact on productivity and applies it to data on 31,399 workers' productivity. The analysis strongly supports signaling theory. The tendency to underreward academic achievement may help explain why American high school students devote less time and energy to learning than do their counterparts abroad. Included are 19 endnotes, 71 references, 3 tables, and a worker evaluation questionnaire. (Author/MLH)

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INFORMATION EXTERNALITIES
AND THE SOCIAL PAYOFF
TO ACADEMIC ACHIEVEMENT

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INFORMATION EXTERNALITIES AND THE SOCIAL PAYOFF TO ACADEMIC ACHIEVEMENT

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I want to express my appreciation to Geoffrey Johnson and David Swarthout of the Northern Test Development Field Center of the Michigan Employment Security Commission for making copies of USES Individual Observation Data File available to me. I would also like to express my appreciation to John Hunter and John Boudreau for their guidance and help in finding studies of the variation of output across workers and to Peter Mueser, Christopher Jencks, Barry Gerhart, George Milkovich, George Jakubson and Ron Ehrenberg for their helpful comments on earlier versions of the paper.

This paper has not undergone formal review or approval of the faculty of the ILR School. It is intended to make the results of Center research, conferences, and projects available to others interested in human resource management in preliminary form to encourage discussion and suggestions.

Abstract

The thesis of this paper is that wage rates and earnings give misleading signals to public and private decision makers regarding the social benefits of certain kinds of education and training (E&T) investments. The misleading signals are a result of the fact that (1) workers and employers prefer employment contracts which either do not recognize or only partially recognize differences in productivity among workers doing the same job and (2) important dimensions of E&T accomplishment -- the skill, knowledge and competencies actually developed -- are often not signaled to potential employers and therefore have limited influence on the allocation of workers to jobs. The result is that there are significant productivity differentials between workers who receive the same pay for the same job and some of these productivity differentials are related to dimensions of E&T accomplishment that are not efficiently signaled.

The paper develops a very simple signaling/implicit contracting model of the labor market. True productivity depends on general intellectual achievement (GIA) and educational credentials but GIA is unobservable, so pay is based on credentials and supervisory assessments of doubtful reliability. As in most signaling models, the labor market tends to overcompensate credentials and undercompensate academic achievement. The next section of the paper refutes the simple wage equals individual MRP assumption by presenting evidence of great variability of productivity across workers paid the same wage and doing the same job. The paper then tests and rejects a weaker hypothesis that can justify an inference that productivity and wage effects of GIA are equal -- namely that deviations of productivity from wages are not correlated with academic achievement. Finally the paper develops a method of estimating the true impact of academic achievement on productivity and applies it to data on the productivity of 31,399 workers.

The analysis provides strong support for signaling theory. As predicted by the theory when workers doing the same job are compared and academic achievement (the unobservable) is controlled, the years of schooling signal is negatively associated with relative productivity. When the schooling signal is controlled, academic achievement has a very strong positive effect on relative productivity. This implies that academic achievement has a larger effect on productivity than it has on wages. Academic achievement produces some private rewards for it facilitates entry into higher paying occupations and promotions into better jobs. These are the effects that are captured by standard wage regressions. In addition GIA has effects not picked up by wage regressions. In each job the individual works he/she is doing a better than average job but not receiving an appreciably higher wage as a result. The results imply that schooling raises productivity primarily by improving academic achievement as it is measured by standard tests. When it does not lead to gains on such tests, the credentials that graduates receive tend to be overcompensated. The second major implication of the results is that academic achievement is substantially under compensated if it is not signaled to the market by a credential. This tendency to underreward academic achievement may help explain why American high school students devote less time and energy to learning than their counterparts abroad.

INFORMATION EXTERNALITIES AND THE SOCIAL PAYOFF TO ACADEMIC ACHIEVEMENT

The thesis of this paper is that wage rates and earnings give misleading signals to public and private decision makers regarding the social benefits of certain kinds of education and training (E&T) investments. The misleading signals are a result of the fact that (1) workers and employers prefer employment contracts which either do not recognize or only partially recognize differences in productivity among workers doing the same job and (2) important dimensions of E&T accomplishment -- the skill, knowledge and competencies actually developed -- are often not signaled to potential employers and therefore have limited influence on the allocation of workers to jobs. The result is that there are significant productivity differentials between workers who receive the same pay for the same job and some of these productivity differentials are related to dimensions of E&T accomplishment that are not efficiently signaled. Another consequence is that the private return to effort in school is considerably smaller than the social return to such effort. This in turn may help explain why American high school students devote less time to learning than their counterparts abroad.

I. The Puzzle: Why Are Labor Market Rewards for Academic Achievement in High School So Modest?

According to the National Commission on Excellence in Education:

> If only to keep and improve on the slim competitive edge we still retain in world markets, we must dedicate ourselves to the reform of our educational system for the benefit of all-old and young alike, affluent and poor, majority and minority. Learning is the indispensable investment required for success in the "information age" we are entering. (p. 7).

Behind their call for higher standards and greater emphasis on academic subjects is the assumption that most jobs require (or soon will require) significant competency in communication, math and reasoning. To what extent does evidence from the labor market support this claim? Are the workers who have these competencies receiving higher wages?

When learning is efficiently signaled by a credential, the answer is an unqualified yes. In 1985 25 to 34 year old male (female) college graduates working full time full year earned 38 (40) percent more than comparable high school graduates and high school graduates earned 23 (16) percent more than high school dropouts. Good educational credentials are also associated with a higher probability of employment.

When learning is not signaled by a credential, the answer is also yes but a highly qualified yes. The labor market rewards for superior academic achievement (controlling for years of schooling) are modest and generally do not appear until many years after the completion of schooling. In Willis and Rosen's (1979) structural model of college attendance and earnings, for example, a one standard deviation increase in the math and reading scores of a high school graduate who did not to go to college lowered the first job's wage by 3.5 percent and raised the wage 25 years later by only 3.5 percent. Other data sets -- Project Talent, Class of 1972, NLS Youth -- yield similarly modest estimates of the private payoffs to academic achievement for those who do not go to college.[1]

Correcting the Willis and Rosen results for measurement error and the restricted range of the test score distribution increases the estimated effect of academic achievement to a modest 2 percent wage gain per grade level equivalent.[2] Consequently, the puzzle remains. Credentials have large effects on earnings even when good measures of what has presumably been learned are included in the regression. Good measures of the skills and knowledge taught in school have small direct effects on earnings when credentials are controlled. One interpretation of this finding is that schooling develops or signals other economically productive talents such as discipline, perseverance and occupationally specific skills and low propensities to quit (Weiss, 1986). A second interpretation is that signals of academic achievement like high school diplomas have value even when actual achievement is absent because employers find it very difficult to measure actual achievement. Either way, it would appear that studying in school and substantially increasing one's achievement test scores yields only modest rewards if credentials do not certify the learning to the world.

Does this imply that the social returns to improvements in general academic achievement are equally small? This requires estimates of the productivity consequences of an increase in academic achievement. The standard approach to such a question is to infer the effect of academic achievement on productivity from its effects on wage rates. This inference is justified by an assumption that individuals are paid their individual marginal revenue products. Is such an assumption justified? Can one conclude that if the wage effects of academic achievement are small, productivity effects are equally small?

The answer provided by the paper is no. The assumption that wages = MRP is shown to be invalid. Evidence is offered that there are large discrepancies between individual productivity (MRP_i) and individual wage rates, (W_i) and that many of these discrepancies are systematically related to academic achievement. This evidence is consistent with signaling and long term contracting theory and inconsistent with a perfect information auction model of employment contracts. The major empirical finding of the paper is that competencies measured by "aptitude" and broad spectrum achievement tests have considerably larger effects on productivity than on wage rates.

The paper is organized as follows: Section 2 of the paper develops a very simple signaling/implicit contracting model of the labor market. True productivity depends on general intellectual achievement (GIA) and educational credentials but GIA is unobservable, so pay is based on credentials and supervisory assessments of doubtful reliability. As in most signaling models, the labor market tends to overcompensate credentials and undercompensate academic achievement. Section 3 refutes the simple wage equals individual MRP assumption by presenting evidence of great variability of productivity across workers paid the same wage and doing the same job. Section 4 of the paper tests and rejects a weaker hypothesis that can justify an inference that productivity and wage effects of GIA are equal -- namely that deviations of productivity from wages are not correlated with academic achievement. The fifth section analyzes the effect of academic achievement and years of schooling on productivity relative to other occupants of the same job. Section 6 reviews evidence on the effect of schooling and relative productivity on within-job relative wage rates.

The analysis provides strong support for signaling theory. As predicted by the theory when workers doing the same job are compared and academic achievement (the unobservable) is controlled, the years of schooling signal is negatively associated with relative productivity. When the schooling signal is controlled, academic achievement has a very strong positive effect on productivity. This implies that academic achievement has a larger effect on productivity than it has on wages. Academic achievement generates private rewards primarily by enabling entry into better schools and by facilitating entry into higher paying occupations and promotions into better jobs. These are the effects that are captured by standard wage regressions. Academic achievement also has effects that are not picked up by a wage regression. In

each job the individual works he/she is doing a better than average job but not receiving an appreciably higher wage as a result. The empirical findings suggest that when academic achievement is not signaled to the labor market by a credential, it tends to be under compensated. Another implication is that if the adult test score/schooling correlation arises because of screening rather than learning, educational credentials are significantly overrewarded, particularly in blue collar, clerical and service jobs. The final section of the paper discusses the implications of these findings for social benefit cost analysis and for educational policy.

II. A Signaling/Implicit Contract Perspective on the Economic Rewards for Academic Achievement

There are a number of reasons why workers and employers may prefer employment contracts which do not pay individual workers their individual marginal product: the unreliability of the feasible measures of individual productivity (Hashimoto and Yu, 1980), risk aversion on the part of workers (Stiglitz, 1974), productivity differentials that are specific to the firm (Bishop, 1987), the desire to encourage coworker cooperation (Lazear 1986) and union preferences for pay structures which limit the power of supervisors. In addition, compensation for differences in job performance may be non-pecuniary -- praise from one's supervisor, more relaxed supervision, or a high rank in the firm's social hierarchy (R. Frank, 1984).

A study of how individual wage rates varied with job performance found that when people hired for the same or very similar jobs are compared, the elasticity of relative starting wage rates with respect to relative productivity is no greater than .08 (Bishop, 1987a). After a year at the firm, the more productive workers were more likely to be promoted, but the elasticity of the relative wage with respect to reported productivity was still quite low. The elasticity was .2 in nonunion firms with about 20 employees and zero in unionized establishments with more than 100 employees and in nonunion establishments with more than 400 employees.

If relative wage rates only partially compensate the most capable workers in a job for their greater productivity, why don't they obtain promotions or switch to better paying firms? To some degree they do, and this explains why workers who score high on tests are both higher paid and more likely to be employed. But the sorting process is not completely effective because employers

cannot accurately predict the future productivity of job applicants or current employees. In addition they usually lack information on "aptitude" test scores or grade point averages that would allow them to predict that component of an employee's productivity that is associated with academic achievement. While college transcripts are commonly requested and used by employers, most employers do not request high school transcripts. Malizio and Whitney's (1984) survey of large employers found that only a handful used high school transcripts to select which applicants to interview, and the majority never requested a transcript at any point in the hiring process. One of the primary reasons for this is that very few employer requests for transcripts are honored. Nationwide Insurance, for example, had over 1,200 job applicants sign requests for high school transcripts in 1982 and received only 93 responses. When the personnel officer asked school staff why transcripts were not forthcoming, he was told they were "too busy". A second reason why employers generally do not use high school transcripts to help make hiring selections is the hiring delays that would result. Schools are often tardy in responding to such requests. Employers, on the other hand, want to make a fast decision. They generally have little notice of openings. In only 23% of the hiring events sampled by the NCRVE employer survey (1982) did the employer have more than 2 weeks notice of the opening. The desire for speed results in 65 percent of job openings being filled within two weeks. Despite limited use of high school transcripts in selecting employees, employers believe that grade point averages are good predictors of future productivity. A policy capturing experiment with a nationwide sample of 750 employers found that employer ratings of completed job applications were more affected by high school grade point average than any other single worker characteristic (Hollenbeck and Smith, 1984).

Referrals by teachers, principals and counselors are another way in which information on academic achievement becomes available to employers. The teachers in occupationally specific programs often provide such referral services but most high school students are not in these programs. Only 3.5 percent of workers report their current job was obtained through the efforts of their school (Rosenfeld, 1975). Most teachers do not have the contacts necessary and do not view developing such contacts to be a part of their job description. Another reason why teacher referrals are uncommon and recommendation letters so bland is that recommenders take a risk if they commit anything negative to paper.

The threat of damage suits by unsuccessful job applicants and the Buckley Amendment have caused school staff to become careful about what they divulge about students.

Tests are probably the best way to evaluate academic achievement. However, the Equal Employment Opportunity Commission's 1971 Guidelines on Employment Testing Procedures prohibit the use of a test on which minorities or women score below white males unless the employer can prove that the test is a valid predictor of performance on jobs at that firm. Each firm proposing to use a test had to do its own validity study separately on blacks and whites (29C.F.R.S607.5(b); Wigdor, 1982). Small firms found the costs prohibitive and did not have enough employees to do such a study. The firm also had to prove that no other test or selection method was available that was equally valid but had less adverse impact. Since there are hundreds of potential selection methods with less adverse impact, the firm was potentially obligated to prove that all of these alternatives were less valid predictors of job performance than the one selected. These guidelines caused many firms to drop tests altogether, while other firms used the test only to screen out the bottom 10 or 20 percent of job applicants, rather than to select those with the highest scores (Friedman and Williams, 1982).

Employers prohibited from using tests of general intellectual achievement (GIA) in their hiring decisions are likely to respond by giving greater weight to visible worker characteristics such as years of schooling which correlate highly with GIA.[4] The use of schooling as a screen results in coworkers having very similar amounts of schooling. Only 20 to 25 percent of the total variance of schooling is within job variance. For test scores in contrast about 44 percent of the population variance is within job variance (Hunter and Hirsh, 1987). Wage regressions estimated in data sets affected by such a prohibition will probably yield higher schooling coefficients and lower test score coefficients.

Assume, for example, competitive labor markets, rational profit maximizing employers and a true relationship between productivity (P^t) and observable credentials (S) and unobservable GIA of the following form:

$$(1) \quad P^t = a_0 + a_1 GIA + a_2 S + u$$

Lacking information on GIA, employers use regressions of measured

productivity (P) on credentials (S) and interview performance (I) for previous new hires to develop rules for selecting new hires and setting initial compensation. General intellectual achievement is related to S and I by $GIA = g_0 + g_1 S + g_2 I + v$ where GIA and I are defined in SD units, $cov(Iu) = 0$ and $g_2 < 1$. Thus, the wage function for new hires is:

$$(2) \quad W' = a_0 + a_1 g_0 + (a_1 g_1 + a_2) S + a_1 g_2 I$$

We assume that this function applies to the 27 percent of the workforce that have less than one year of tenure (Horvath, 1981). For the other 73 percent compensation is set equal to a productivity expectation (P) that is based on credentials and a supervisory assessment (R). This supervisory assessment is an imperfect measure of a weighted average (P) of past productivity levels (P_n, . . P_0) calculated using weights, $w_t = (w_n, \ . \ . \ w_0)$.

$$(3) \quad R = P + \epsilon = \sum_{0}^{n} w_t P_t \ / \ \sum_{t=0}^{n} w_t + \epsilon$$

The compensation schedule will be:

$$(4) \quad W'' = P_{n+1} = c_0 + c_1 R + c_2 S$$

Supervisor ratings correlate only .6 with each other and .43 with work sample measures of job performance and repeated measurement increases reliability only marginally (Hunter, 1983; King, Hunter and Schmidt, 1980). This means that the variance of ϵ is considerable. Hashimoto and Yu (1980) have examined optimal pay structures when the measure of productivity is unreliable and have demonstrated that the tendency to compensate higher productivity with higher pay diminishes with the decline in the reliability of the productivity measure. The coefficient on supervisory assessment (c_1) in the wage function for long term employees will consequently be considerably less than 1.[4]

Now enter an analyst whose assignment is to uncover the true relationship between schooling, GIA and productivity. For a large sample of workers the analyst collects data on wages, credentials and GIA (adult test scores) and estimates the following regression:

$$(5) \quad W = b_0 + b_1 GIA + b_2 S$$

Since the aggregate wage function is an average of (2) and (4) with weights .27 and .73 respectively, the resulting coefficient estimates will be:

$b_1 = a_1(.27g_2 + .73c_1) < a_1$ and $b_2 = .27(a_1g_1 + a_2) + .73(c_1a_2 + c_2) > a_2$

These results correctly characterize the private payoffs to the two dimensions of schooling. However, they do not correctly characterize the pattern of social returns. An analyst who made the standard assumption that W=P would obtain downward biased estimates of the effect of academic achievement on productivity and upward biased estimates of the effect of credentials on productivity. The evidence supporting this assertion and the empirical relevance of signaling and implicit contracts theory is presented in the sections that follow.

III. Are There Important Discrepancies Between Wage Rates and Individual Marginal Revenue Products?

A direct test of $W_1 = P_1$ and of whether wage equations yield biased estimates of GIA's effect on productivity will be presented. A good way to conduct the test is to sample workers who do the same job and are paid the same wage and measure their output directly. If output varies substantially in such samples, $W_1=P_1$ must be rejected.

A search for studies of output variability yielded 27 published and 7 unpublished papers covering 69 distinct jobs.[5] Their results are summarized in column 3 and 4 of Table 1 (a description of methods used to estimate CVs and the sources can be obtained from the author). For a great many occupations physical measures of output or gross sales data were the basis of these estimates of the standard deviation of productivity. The average ratio of the standard deviation of output to mean output, coefficient of variation or CV, was 50 percent for high level sales workers, 38 percent for sales clerks, and 20 percent for clerical workers and hourly paid semi skilled factory workers. For other occupations estimates of output variability were obtained from managers and industrial engineers who supervise individuals in the occupation. The average CV was 36 percent for technical jobs, 33 percent for managerial jobs, 54 percent for plant operators and 27 percent for other craft workers. Coefficients of variation appear to be higher in jobs with greater cognitive complexity and in sales jobs.

When a firm expands by hiring extra workers, it incurs significant fixed costs. It must rent space, buy equipment, hire supervisors and recruit, hire, train, and payroll the additional production workers. If output can be increased by hiring more competent workers, all of these costs can be avoided and the firm's capital becomes more productive. These factors tend to magnify the

effects of work force quality on productivity. They imply that the ratio of the standard deviation of worker productivity in dollars (SD$) to average worker compensation is much larger than the productivity CV for that job (Klein, Spady and Weiss 1983; Frank 1984).

Estimates of productivity standard deviations (SD$) in 1985 dollars are reported in column 4 of the table. In many cases the original study of output variability made no attempt to estimate SD$'s, so the estimate has been calculated from the CV. The estimates of SD$ were derived as a product of the CV, the mean compensation for that job and the ratio of value added to compensation for that industry (for manufacturing as a whole this ratio is 1.63). The value added to compensation ratio in retailing and in real estate was much too high to be used as an adjustment factor. So for all sales occupations it was assumed that SD$ = CV times average compensation. While the SD$'s are substantial even in low wage clerical jobs, there is clearly a positive correlation between average wage levels and SD$'s. Except for the higher level sales personnel and one of the managerial occupations studied, these workers were not paid commissions or bonuses.

While it is possible to debate the accuracy of specific estimates, one would have to take the extreme view that SD$ is almost zero before the basic conclusion that workers paid the same wage are often significantly different in productivity would change. This implies that the $W_i=P_i$ assumption cannot possibly be true.

IV. Are Discrepancies Between Wage Rates and MRP Positively Correlated With Academic Achievement?

There is, however, a weaker assumption that would make the standard wage equation an unbiased estimator of GIA's impact on productivity, namely:

(6) $W_i = E(P_i | GIA_i, S_i, X_i)$. where i indexes individuals

This also is testable in data containing measures of P, GIA, S and other characteristics of the worker such as gender, ethnicity and experience (X) for people doing the same job and paid the same wage. If employers know GIA and adjust pay accordingly, then in samples of workers paid the same wage there should be no significant correlation between GIA and P conditional on S and X. It is possible to test this hypotheses, for industrial psychologists have conducted literally hundreds of studies (covering hundreds of thousands of workers) of GIA's association with relative productivity in samples of job

incumbents. Most of these studies have been conducted in samples of workers whose hourly wage depended on seniority and not performance.

The first column of Table 1 presents average correlations between GIA tests and supervisory ratings of job performance from Ghiselli's (1973) comprehensive review of published and unpublished studies of the validity of GIA tests.[6] The second column of the table presents correlations from the GATB Manual (Department of Labor, 1970) and from other recent meta analysis. Clearly there is a significant positive correlation between GIA test scores and job performance in a great variety of jobs. The strength of the association is apparently related to the cognitive demands of the job, for the raw validities are higher for white collar and skilled blue collar jobs than for semiskilled factory work, transportation equipment operatives and retail sales clerks. Analysis of data sets which have better measures of job performance (work sample measures rather than supervisory ratings) find even stronger relationships between GIA and job performance (Hunter, 1983). Except for sales representatives, and some jobs paid on a piece rate, there was minimal variation of wages in these samples not related to seniority. The comparable correlations for grade point averages (.11) and for years of schooling (.10) are much smaller (Hunter and Hunter, 1984).

In summary, there is considerable evidence that workers who do the same job at a firm and are paid a wage that depends on seniority only, are often quite different in productivity and these differences in productivity are often correlated with the employee's measured academic achievement. These two results imply that GIA has larger effects on productivity than on wage rates. A method of measuring the effect of GIA and years of schooling on the discrepancy between a worker's productivity and his or her wage will now be described.

V. The Effect of Academic Achievement on a Worker's Productivity Relative to Coworkers

Wage and earnings models such as equation (5) are the starting point of any effort to calculate the productivity consequences of worker characteristics such as schooling and academic achievement. If used alone, however, they yield biased estimates of the productivity consequences of academic achievement. To correct the bias, we need an empirical model of the discrepancy between an individual's productivity and wage rate which can be added to earnings functions like equation 5. The sum of the coefficients from the two separate equations

is the estimate of the vector $\underline{a}$ in equation 1, the true productivity relationship.

The discrepancy between a worker's productivity and wage rate can be decomposed into 3 elements:

$$(7) \quad P_{i,j}-W_{i,j} = (P_{i,j}-P_j) - (W_{i,j}-W_j) + (P_j-W_j)$$

The first term is the worker"s "relative productivity", the deviation of the "i"th worker's marginal revenue product net of current required training costs ($P_{i,j}$) from the marginal revenue product net of training costs (P_j) of the average incumbent in the „j"th job at the firm. Evidence on how relative productivity is related to worker characteristics is presented below. The second term is the worker's "within-job relative wage", the deviation of an individual's wage from the mean for that job at the firm. Evidence on how the within-job relative wage relates to worker characteristics is presented in section 6. The last term is the difference between the marginal revenue product of the <u>average</u> incumbent in the job (P_j) and the average wage for the job (W_j). An examination of this last term would require direct measures of the marginal revenue product of work groups that are comparable across jobs and across firms. Such data are not available. It is assumed that P_j-W_j summed over a worker's life cycle is uncorrelated with schooling and GIA.[7] The paper focuses its analysis on the first two terms of equation 7.

<u>Analysis of GATB Validation Studies</u>

Data on the relative productivity of a large and reasonably representative sample of workers is available from the US Employment Service's program for revalidating the General Aptitude Test Battery (GATB). The Employment Service's Individual Data file contains data on job performance, the 9 GATB "aptitudes" and background data on 36,614 individuals in 143 different occupations. Professional, managerial and high level sales occupations were not studied but the sample is quite representative of the rest of the occupational distribution. It ranges from drafters and laboratory testers to hotel clerks and knitting-machine operators. A total of 3052 employers participated. Since a major purpose of these validation studies was to examine the effects of race and ethnicity on the validity of the aptitude test battery, the firms that were selected tended to have an integrated workforce in that occupation. Firms that used aptitude tests similar to the GATB for selecting new hires for the job

being studied were excluded. The employment service officials who conducted these studies report that this last requirement did not result in the exclusion of many firms.

Each worker was administered the GATB test battery and asked to supply information on their age, education, plant experience and total experience. Plant experience was defined as months working in that occupation for the current employer. Total experience was defined as months working in the occupation for all employers. The "Final Criterion" used by these studies was generally the sum of scores from two administrations of the Standard Descriptive Rating Scale. In some cases different supervisors rated the same individual but in most cases the same supervisor was readministered the scale after an interval of about 2 weeks. This rating scale (available from the author), obtains supervisory ratings of 5 aspects of job performance (quantity, quality, accuracy, job knowledge and job versatility) as well as an "all around" performance rating. Some studies employed rating scales specifically designed for that occupation and in one case a work sample was one of the job performance measures. None of the studies used ticket earnings from a piece rate pay system as the criterion. For this analysis studies which used course grades or tests of job knowledge as a criterion were excluded. Firms with only one employee in the job classification were excluded, as were individuals whose reported work experience was inconsistent with their age.

Academic achievement is proxied by two GATB composites, G and N.[8] General Intellectual Achievement (G) is an average of normalized scores on a vocabulary test, an arithmetic reasoning test and a 3-dimensional spatial relations test. The mathematical achievement index (N) is an average of normalized scores on the same arithmetic reasoning test and on a numerical computations test. Both were put into a Population SD metric by dividing by 20.

Our objective is to explain variations in performance across workers doing the same job at the same firm. Because wage rates, average productivity levels and the standards used to rate employees vary from plant to plant, mean differences in ratings across establishments was assumed to have no meaning. Only deviations from the mean for the establishment were analyzed. The variance of the job performance distribution was also standardized across establishments by dividing $(R^{m}_{i,j}-R^{m}_{j})$ by the standard deviation of performance calculated for that firm (or 3 if the sample SD is less than 3).[9] The model that was fitted

to the data was the following:

$$(8)\quad \frac{R^m_{ij}-R^m_{kj}}{SD(R^m_j)} = R_{ij}-R_j = \theta_0 + \theta_1 GIA_{ij} + \theta_2 S_{ij} + \theta_3 X_{ij} + v_i$$

where GIA_{ij}, S_{ij} are the GIA and schooling of the individual and X_{ij} is a vector of individual characteristics which includes gender, Black, Hispanic, age, plant experience, total occupational experience and their squares.[10]

Table 2 presents estimates of equation 8 that were estimated in the full data set. The GATB achievement tests are clearly strongly correlated with relative job performance. Adding controls for race, gender, schooling, age, plant experience, total occupational experience and their squares does not significantly reduce the magnitude of this relationship. In model 3 a one population standard deviation test score differential on both tests results in a relative job performance differential of 16.9 percent of a SD(R), a within firm standard deviation of the job performance rating. In contrast, schooling has a significant negative direct effect on relative job performance when measures of actual achievement are controlled. If they do not result in higher test scores, four additional years of schooling appear to reduce relative job performance by 9.6 percent of an SD(R). The negative effect of schooling together with the large positive effects of measured academic achievement is strong confirmation of the empirical relevance of signaling and implicit contracts theory.

These results, however, do not support Ivar Berg's (1970) claim that educated workers are systematically overpaid. Workers with high amounts of schooling are not reported by their supervisors to be less productive than others in their job. When G and N are not included in the model, schooling no longer has a negative effect on relative job performance. When equation 8 is estimated in the full sample, the coefficient on schooling is .006 (t=1.97) if schooling is entered alone and .011 (t=3.60) if gender, race, Hispanic, age, plant experience and occupational experience are controlled but test scores are not. When equation 1 is estimated using fixed effects, the coefficient on schooling is .009 (t=2.48) if schooling is entered alone and .029 (t=7.79) if gender, race, Hispanic, age, plant experience and occupational experience are controlled but test scores are not.

Willis and Rosen found that academic achievement measured while the individual was in the armed forces had a larger impact on the wages of those

with some college education than those who did not go to college. This interaction was tested by interacting the deviation of G from its mean with a dummy for more than 12 years of schooling. The results presented in row 4 of Table 2 reveal that academic achievement's effect on productivity is larger for college educated workers than for those with 12 or fewer years of schooling. A one population standard deviation achievement differential on both G and N raises a college educated worker's productivity by .205 SD(R)'s and a noncollege educated worker's productivity by .152 SD(R)'s.

It is well documented that the earnings payoff to academic achievement tends to grow with age (Hauser and Daymont 1977; Taubman and Wales 1974). One explanation of this pattern is that academic achievers tend to take jobs that offer a greater amount of on-the-job training and/or receive higher rates of return on their on-the-job training. A second explanation of the pattern is that employers may be better informed of the productivity of older workers. Promotions and turnover would have had more time to sort the older individual into a job in which wage truly equaled marginal product. An extreme version of this second scenario predicts that academic achievers should after a time have been promoted into a job in which they are no longer perform better than the average for that job.

This hypothesis was tested in our data by specifying interactions between age and G, between total occupational experience and G, between plant experience (tenure) and G and between plant experience and years of schooling. It was hypothesized that coefficients on the G interactions would be negative. When all four interactions were entered simultaneously, all were statistically insignificant. The tenure-G interaction had the largest negative coefficient so the model was reestimated with only the tenure-G interaction. The tenure-G interaction was equal to G deviated from its mean multiplied by a dummy for tenure greater than 59 months (the approximate mean for the sample). Results are reported in row 5 of Table 5. The coefficient on the tenure-G interaction is negative and significant at the 5 percent level. This implies that there is some tendency for the discrepancies between productivity and wage rates that are correlated with G to be greater early in a worker's tenure at a firm. Presumably promotions and selective attrition sort academic achievers into better jobs in which they are somewhat less likely to be substantially more productive than their peers. However, the magnitude of the interaction effect is quite

small. The effect of G and math achievement on the relative productivity of those with more than 5 years of tenure is only about 15 percent less than G and math achievement's effect on those with fewer than 5 years of tenure. Consequently, the extreme version of scenario 2 is not supported by the data. This suggests that greater access to OJT and higher rates of return on OJT investments are part of the reason why the earnings payoff to academic achievement increases with age.

Academic achievement helps a worker learn new and complicated jobs faster and more thoroughly. It should, therefore, raise the productivity of on-the-job training. Large companies typically offer more training than small companies (Bishop 1982), so it is hypothesized that GIA will have a larger effect on relative productivity at larger companies. To test this hypothesis an interaction variable was defined by multiplying G by the log of one plus the number of individuals in the occupation at the establishment divided by 10. The results of including this interaction are presented in row 6 of Table 5. The coefficient on the interaction variable is highly significant, so the hypothesis is supported. But the magnitude of the interaction effect is rather modest. A one POPSD increase in both G and N raises relative productivity by .158 SD(R) if the company has only 9 people in the job and by .174 SD(R) if the company has 100 people in the job.

Interactions with occupation were tested by estimating separate models for 5 major categories of occupations: technicians, clerical, high skill blue collar, low skill blue collar and service workers. The results of these estimations are presented in Table 3. Mean levels of academic achievement vary greatly across occupations. In column 6 of the table we see that the test scores of low skill blue collar workers and service workers are about 60 percent of a standard deviation below those of technicians, clerical workers and high skill blue collar workers. The results reported in column 1 and 2 of the table demonstrate that academic achievement has substantial effects on job performance in all occupational categories even those requiring the least skill. The partial correlation of schooling with job performance was significantly negative in all of the blue collar occupations, non significantly negative for clerical workers and essentially zero for technicians. This suggests that the tendency to over reward credentials may be confined to blue collar jobs.

It is often claimed that once some minimum level of academic achievement

is reached, higher levels of achievement make no further contribution to job performance. This hypothesis was tested by adding the square of G to the models presented in Tables 2 and 3. The hypothesis was rejected. Only 1 of the 6 coefficients was negative. None of the coefficients were statistically significant at even the .20 level on a two tail test.

The true relationship between academic achievement and productivity is in fact stronger than the results reported above, for they have been attenuated by measurement error. The alternate form reliability for GIA and math achievement are .875 and .845 respectively (Department of Labor, 1970). Measurement error in schooling has a variance of about 1.0 (Bishop 1976; Jencks et al 1979). The upper bound on the reliability of job performance measures like the Standard Descriptive Rating Scale has been found to be .6 (King, Hunter and Schmidt, 1980). Therefore, the following measurement model was appended to equation 8.

$$
\begin{aligned}
& G_{ij} = GIA_{ij} + v_2 \\
9) \quad & N_{ij} = GIA_{ij} + v'_2 \\
& YRED_{ij} = S_{ij} + v_3 \\
& X_{ij} = X^t_{ij} + v_4
\end{aligned}
$$

$$
10) \quad R_{ij}-R_j = \sqrt{r_p}\,p^t_{ij} + v_5 = \sqrt{r_p}[(P^t_{ij}-P^t_j)/SD(P^t_j)] + v_5
$$

where except for $r(v_2,v'_2) > 0$; v_1, v_2, v'_2, v_3, v_4 and v_5 are uncorrelated, r_p is the reliability of the job performance measure, $(P^t_{ij}-P^t_j)$ is the deviation of "i"s true productivity in dollars from the mean for that job and X^t_{ij} is a vector of the true values of individual characteristics like gender, ethnicity, age, tenure and occupational experience. For independent variables, the measurement model makes the standard assumption that measurement errors are uncorrelated with the true values of the variables, with each other and with equation error.[11] The measurement model for productivity is the standard model employed by industrial psychologists. It assumes that the rating of relative job performance, $(R_{ij}-R_j)$, is a cardinal measure of productivity and that its relationship with true productivity is linear. The metrics of $R_{ij}-R_j$ and of p^t_{ij} have been chosen to give them a mean of zero and unit variance.

The LISREL program was employed to obtain maximum likelihood estimates of the full system of equations. Estimates of the coefficients for GIA and

schooling are presented in line 7 of Table 2. Taking into account measurement error greatly increases the estimated negative effect of schooling. Controlling on achievement, an additional year of schooling is associated with a reduction in relative productivity of .079 SD(p^{L}). The direct effects of academic achievement on productivity were large before correcting for measurement error; now they are double their previous level. A one population standard deviation increase in GIA results in a .321 SD(p^{L}) improvement in true job performance.

Except for the different metric of the dependent variable, these effect estimates are intended to be comparable to structural coefficients from wage equations estimated in representative samples of the full population. Since, however, the data do not include professional, managerial and high level sales jobs, the results do not refer to the top end of the skill distribution and may be subject to selection bias. Adding these occupations to the data base might make the coefficient on schooling less negative. Since, however, fixed effects estimates of equation 1 find that GIA's validity in predicting job performance is greater in the more cognitively demanding occupations, the positive effect of GIA on relative productivity might become even larger if these higher level occupations had been included.

Analysis of PACE Validation Studies

The second data set to be examined are three studies conducted by the Office of Personnel Management to validate the written test portion of the Professional and Administrative Career Examination (PACE). The jobs studied are Internal Revenue Officers (Oleary and Trattner, 1977), Social Insurance Claims Examiners (Trather, Corts, Van Rijn and Outerbridge, 1977) and Customs Inspectors (Corts, Muldrow and Outerbridge, 1977). The prime reason for analyzing these data is that these studies have better criterion measures than are usually available: a carefully constructed work sample as well as the standard ratings by first line supervisors. The means, standard deviations and the correlation matrix for GIA (the weighted Test 500), schooling, years of experience, work sample and supervisory ratings was obtained from the published reports and by correspondence with Alice Outerbridge, an author of two of the studies. Information on the reliability of the criterion measures, and the test comes from these reports and from Hunter (1983).

The estimated coefficient for the underlying latent variables, their t statistics and the coefficients of determination of the models are reported

in table 4. Since the analysis is conducted in a highly selected sample of workers in just one job, it is a fixed effects estimate of equation 1, not an estimate of equation 8. Consequently, Table 4 presents (firm) fixed effects estimates of a_1 and a_2 not estimates of O_1 and O_2 as in the case of table 2. The results may be summarized as follows:

1. The effects of GIA on job performance are substantial.
2. GIA's estimated impact on measured productivity is higher when the more accurate work sample approach to measuring productivity is used.
3. The Peter principle apparently operates in these jobs. The most competent are promoted to better jobs so tenure has a small negative partial association with productivity.
4. When GIA is controlled, the direct effect of schooling on productivity is very small and often negative.
5. The estimated effect of GIA on productivity is not appreciably reduced by including tenure and years of schooling as controls.

Calculations of the Effect of GIA on Relative Productivity in Dollars

Up to this point the effect of GIA and years of schooling on job performance ratings and on true productivity have been reported in standard deviation units not in dollars or percentages. While the findings that GIA is underrewarded and that schooling is overrewarded (if GIA has not improved) do not depend on such a translation, the substantive importance of these findings depends on the implied dollar magnitudes so it would be useful to have an estimate of their magnitude. As Brogden (1949) points out, this can be simply achieved by multiplying the estimated effect of a variable on true productivity in standard deviation units by an estimate of the standard deviation of true productivity across workers, $SD(P^t_j)$, obtained from another source. Most of the studies of the variability of output across workers, SD$, summarized in Table 1 were efforts to obtain estimates of $SD(P^t_j)$ (a more complete description of the studies is provided in Appendix A which is available from the author). The industrial psychology literature contains hundreds of studies estimating the utility (in dollars) of methods of selecting new hires and other personnel policies using the approach suggested by Brogden. All of these studies assume that $SD\$ = SD(P^t_j)$, so this is the assumption employed in the calculations below.[12]

For the occupations included in the GATB revalidation data base the weighted average SD$ is $10218 or 41.1 percent of mean compensation in these occupations.[13] Multiplying these values by -.079, the estimated effect of a year of schooling on p^{c}_{ij}, the implied effect of schooling (holding GIA constant) on relative productivity in dollars is to reduce it by $807 or 3.2 percent of compensation in these occupations. The relative productivity effect of a Population SD of GIA is much larger--$3280 or 13.7 percent of compensation in these occupations.

The effects of academic achievement on rated job performance appears to be quite large. An information externality is implied, however, only if within job relative wages do not respond to academic achievement in a like manner. The next section of the paper examines the extent to which relative wages of individuals doing the same job depend on productivity or academic achievement.

VI. The Effects of Academic Achievement on Within-Job Relative Wages.

This section of the paper examines the determinants of within-job relative wage rates, $W_{ij}-W_{j}$. Probably the most important single determinant of within job relative wage rates is tenure. In many jobs tenure is the only source of pay differentials. In 52 percent of plant jobs and 14 percent of office jobs at establishments with more than 50 employees, wage rates either do not vary or vary only with tenure.(Cox 1971) This implies that at these establishments there is a tendency to underreward academic achievement.

What about the remainder of establishments which set pay individually, or use merit pay plans with individual incentives? Wages and academic achievement might be positively associated at these establishments. We would expect some association because employers know the schooling of their employees and tend to make higher wage offers to those with greater schooling. Analysis of the NCRVE employer survey (a data set which lacks test score measures) found that wage rates are significantly higher for those with greater schooling even when one is comparing two workers doing the same or a very similar job. Each additional year of schooling was associated with wage rates being 1.1 percent higher (t=2.87) at the start and 1.2 percent higher (t=2.23) at the end of a year (Bishop 1987). Regressions predicting a ratio scale measure of reported productivity in this same data set find that years of schooling had no effect on initial productivity but that after one year at the firm, each additional

year of schooling was associated with about 1 percent higher productivity (Bishop, et. al. 1985). We know, however, from the analysis of GATB and PACE data that the correlation between schooling and productivity relative to one's coworkers arises from their common association with GIA and math achievement.

For college graduates there is still another way in which educational achievements may be credentialed--the reputation of the college and the GPA achieved at this school (recall that college transcripts are available to employers). David Wise (1975) found that college selectivity and GPA had a significant effect on promotions and wage increases of professional and managerial employees at Ford Motor Company.

These findings on the effects of schooling, GPA and college selectivity confirm that credentials signaling academic achievement are rewarded. However, correlations between years of schooling, GPA and scores on GIA tests are not all that high. In Project Talent data, for example, scores on achievement tests taken in high school correlated only .38 with high school grades (Jencks and Crouse 1982). The correlation between schooling and adult test scores is .42 in the GATB data, .473 in the PSID and .7 in IQ standardization samples(Jencks et al 1979; Matarazzo 1972). Consequently, these findings leave open the question of whether academic achievement not signaled by a credential is rewarded. In order for this to occur the firm would have to base wage offers on either a test score or on job performance. The employers in the GATB validation studies had not administered tests to their employees, so wages could not have been based on test scores.[14]

Firms do base wage decisions on job performance but the magnitude of the wage response is small compared to the magnitude of the productivity differentials that arise between people doing the same job. CPS surveys reveal that only 1.2 percent are paid on a piece rate basis and only 1.9 percent are paid on a pure commission basis (Flaim 1979). Analysis of the NCRVE employer survey found that the elasticity of starting wage rates with respect to the ratio scale measure of relative productivity was .08 or less and that after one year on the job this elasticity had risen no higher than .22 (Bishop 1987).

In Medoff and Abraham's data current job performance ratings have only small effects on the current wage rate of senior professional and managerial workers when job classification is held constant. A one standard deviation improvement in a worker's rated performance raised wage rates by 1.9 percent at company

A, by 1.2 percent at company B and by 3.5 percent at Company C (Medoff and Abraham 1980a, 1980b). After a rough correction for measurement error in job performance, these estimates become 3.2, 2.0 and 5.8 percent respectively.[15]

The ratio of SD$ to mean compensation in technical and administrative occupations is approximately one half. Making the assumption that is conventional in the industrial psychology literature that SD$=SD($P^{*}_{j}$), the share of a productivity differential that accrues to the worker in higher wages can be calculated by dividing the percentage wage differential resulting from a one SD performance differential by 50 percent. This share appears to be under 15 percent in the Medoff/Abraham data.

The response of wages to multiyear averages of relative productivity levels is likely to be higher. A study by Gerhart and Milkovich (1987) of professional and managerial workers in a large diversified manufacturing firm found that while a one SD differential in 1980 performance ratings was associated with only a 2.8 percent differential in 1980 wages, consistently high ratings generated larger wage increases and a more rapid climb up the firms's job heirarchy. A one SD differential in average ratings during the 1980 to 1986 period resulted in a 5.6 percent larger wage increase over the 6 year interval. Nevertheless, the increment in average earnings over the course of the six year period appears to be only about 18 percent of a true productivity increment during those 6 years. [Note that in many cases the increased wages are the result of promotions into higher job categories.] If we assume a 4 percent yearly risk of permanent separation and a 6 percent real discount rate, the present value of the lifetime earnings gain from a one SD differential in true performance in a given year is about 15.5 percent of one year's compensation. This calculation would seem to imply that even in the long run only about 30 percent of the unanticipated ex post productivity contributions of a worker at this firm accrued to the worker in higher lifetime earnings.[16]

It is, therefore, fair to conclude that the personal rewards for academic achievement arise primarily from obtaining or being promoted into better jobs, not from being paid more in a given job. This is especially true in the clerical and blue collar jobs that predominate in the GATB validation data set. In blue collar and clerical occupations, measures of academic achievement such as test scores and GPA have almost no effect on wage rates when schooling is controlled. Taubman and Wales' (1974) analysis of NBER Thorndike data, for instance, found

that in these occupations a one standard deviation test score differential raised earnings by only 1.3 percent for those in their early 30's and by 1.9 percent for those in their middle 40's. In High School and Beyond followups of those who did not go to college, correlations between indicators of academic achievement and wage rates are negative for unskilled and semiskilled blue collar workers of both sexes and for male clerical and retail sales workers. For female clerical and retail sales workers the correlations are positive, but the implied effect of academic achievement on wage rates is small.

One is forced to conclude that for these occupations academic achievement's effects on within-job relative wage rates are significantly smaller than its effects on relative productivity in the GATB validation data. Consequently, the effect of GIA on discrepancies between productivity and wage rates is almost as large as its effect on productivity and, therefore, are of significant size.

VI. Summary and Implications

While theory states that wage rates and earnings differentials are a good proxy for differentials in marginal revenue product when different firms, jobs and occupations are being compared, signaling and implicit contracts theory implies that no such prediction can be made when coworkers with the same job assignment are being compared. The results presented above establish that productivity differences between workers who do the same job and are paid the same wage at a firm are often quite large and are correlated with academic achievement. These results provide support for the practical significance of signaling theory. They also provide support for the proposition that academic achievement is underrewarded in the US labor market. The major qualifications that must be added to this last conclusion are (a) it depends on a maintained assumption that a lifetime average P_j-W_j is uncorrelated with GIA_{ij} and S_{ij}, (b) it is based on analysis of a data set that does not contain the most cognitively demanding jobs and (c) estimates of the magnitude of these effects are sensitive to the maintained assumption that $SD\$ = SD(P^L_j)$. Nevertheless, these findings have significant implications both for policy analysis and policy.

Implications for Policy Analysis and Research

If something as easy to measure as academic achievement generates uncompensated productivity differentials, other difficult to signal educational achievements probably have the same effect. This implies that when educational achievements are not well signaled to employers, standard evaluation techniques

which compare the earnings of randomly assigned treatment and control groups may yield worthless or biased estimates of the social benefits of the program. When followups last only a year or so, the conventional approach more nearly measures the reputation of a program's graduates, than it measures the true impact of the educational experience on productivity.

The correlation between reputation and reality is likely to be low for programs in operation only a short time, for programs that change frequently or have high staff turnover, for special programs with different entry and graduation criteria from those prevailing elsewhere in the educational institution and for programs that have done a particularly good or poor job of marketing their graduates.

The paper has attempted to show that evaluations of educational and training programs need not and should not be confined to examining wage and earnings effects. This is an essential first step but a second step is required as well. The second step involves comparing the productivity of graduates of the program to other comparable workers in the same job who are paid the same wage. If the second study is done well, the total social benefits of the educational program can be obtained by simply adding the productivity and wage effects together. This method has been applied to a growth accounting analysis of the effects of the test score decline (Bishop 1987b).

Implications for Education and Training Policies

This paper confirms one of the central predictions of signaling theory: when workers in the same job are compared and academic achievement is controlled, schooling is negatively correlated with a worker's relative productivity. The lack of information on achievements in school means that hiring selections and starting wage rates often do not reflect the competencies and abilities individuals developed in school. Instead, these decisions are based on observable characteristics such as educational credentials that in the United States are very imperfect signals of the competencies that cannot be directly observed. If we assume, as the industrial psychology literature appears to, that $SD(P^{L}_{J})$ equals SD$, a year of schooling appears to be associated with reductions in relative productivity equal to 3.2 percent of compensation when GIA is held constant. Since a year in high school raises earnings only 5 to 6 percent when family background and adult test scores are controlled (Jencks et al. 1979), the direct effect of a year of secondary school on productivity

(that which is not mediated by test score gains) would appear to be only 2 to 3 percent. If schooling has a positive effect on within-job wage differentials when GIA is controlled, the direct effect of schooling is even smaller.

The results also have implications for the screening theory of education. When schooling does not enhance productivity but only signals inherent productivity and firms need not know who is most able to realize the benefits of highly able employees, the social benefits of schooling are much smaller than its private rewards (Spence 1973; Stiglitz 1975). If GIA's correlation with schooling is entirely due to selection and not to learning, the measurement model results suggest that schooling's effect on productivity is 3.2 percent per year less than is implied by standard wage equations. It would appear there may be a problem of "overschooling". The extent of the problem depends on the extent to which schools select for talent rather than developing it.

Up to this point, it has not mattered whether G and N measured an inherited trait or a competency acquired in school. Now it does. A definitive treatment of this controversial topic is beyond the scope of this paper, but a quick review of some of the important findings related to the issue is provided below. There appears to be considerable evidence that scores on "aptitude" tests are significantly affected by environmental factors such as schooling. For example,: (1) school attendance raises scores on "aptitude" tests (Lorge 1945; Husen 1951; Department of Labor 1970), (2) population means on IQ tests (the Stanford-Binet, the Wechsler and the Army Alpha) rose by more than a standard deviation between 1918 and 1965 (Bishop 1987b; Flynn 1984), (3) scores of Japanese, French, German, and Dutch youth on IQ and army entrance exams rose by more than a standard deviation in the postwar period (Flynn 1987), (4) achievement gaps between white youth and Black and Hispanic youth have declined over the last 20 years (Koretz 1986), and (5) broad spectrum achievement tests correlate almost as highly with verbal and mathematical "aptitude" tests as alternate forms of the same test correlate with each other.[18] If as argued above, the correlation between adult GIA and schooling reflects learning more than selection, the screening bias in estimates of returns to schooling becomes smaller and the externalities of schooling probably outweigh any "overschooling" effect.

Years in school is not, however, the only dimension of educational investment. The effort exerted per year is equally as important. Consequently, an even more important implication of signaling models (one that has been

neglected by the literature) is that whenever credentials are awarded for years in school and learning is difficult to verify by other means, the private rewards for effort and learning will be reduced, and students will underinvest in this dimension of their education. The distortions that result from the absence of good signals for academic achievement appear to be very significant. A one POPSD improvement in GIA generated by studying hard appears to increase a worker's expected productivity relative to coworkers in the same job by $3280 or 13.7 percent of average compensation. Only a small part of this increase in relative productivity is apparently captured by the worker in the form of higher within-job relative wage rates.

Can we expect achievement gains resulting from harder studying or better teaching to raise wages and relative productivity by the amounts implied by the coefficients on GIA in equation 5 or equation 8? Those who believe that the "G" and "N" aptitudes of the GATB are in fact measures of inherited learning ability might argue to the contrary that productivity is an outcome of on-the-job learning, not in-school learning, and that these tests measure inherited learning ability, not outcomes of schooling that improve job performance. In this view, G and N are good measures of inherited learning ability because everyone receives roughly equivalent instruction in the material covered by the test, so differences in knowledge at the end of instruction primarily reflect differences in inherited learning ability. This view, however, does not withstand scrutiny.

Many of its key predictions are contradicted by data. 1) If it were true, we would expect childhood IQ tests to predict adult labor market success just as well as adult IQ tests. In fact, when adult IQ tests compete with childhood IQ tests, it is the adult test, not the childhood test, which has by far the biggest effect on labor market success (Husen, 1969). (2) In addition, we would expect less "culturally loaded" non-verbal IQ tests to be equally good predictors of labor market success as tests of reading and writing skills. In fact, a study of Kenyan workers has found that wages were significantly affected by literacy but not by non-verbal IQ (Brossiere, Knight and Sabot, 1985). (3) Furthermore, we would expect education obtained abroad in non-English speaking countries to be just as good a signal of high IQ (and therefore just as good a predictor of wage rates in the U.S. economy) as education obtained in the U.S. or English speaking countries. In fact, a year of schooling obtained in

a non-English speaking country has a much smaller effect on wage rates than a year of schooling obtained in the U.S. or another English speaking country. (Chiswick, 1978). (4) Finally, we would expect that controlling for genotype IQ (e.g. by comparing identical twins) would reduce the effect of test scores on labor market success to zero. Since siblings are genetically similar, we would expect IQ effects to diminish when siblings are being compared. In fact the effect of IQ (measured while in school) on labor market success is actually greater when brothers are compared than in standard cross section regressions (Olneck 1977).

These findings suggest that the associations between the "G" and "N" aptitudes of the GATB and relative productivity arise primarily because the tests measure skills and competencies that contribute to productivity and not an inherited learning ability and, therefore, that the coefficients obtained on GIA when equations like 5 and 8 are estimated provide reasonable estimates of the true causal impact of achievement gains that result from better teaching or studying harder. Consequently, it would appear that signaling problems diminish considerably the private economic payoffs to raising the quality of education and to studying hard while in school.[18]

The tendency to underreward effort and learning in secondary school may be a peculiarly American phenomenon. Grades in school are a crucial determinant of which employer a German youth apprentices with. Top companies in Japan and Europe often hire lifetime employees directly out of secondary school. Teacher recommendations, grades and scores on national and provincial exams have a significant impact on who is hired by the more prestigious firms (Rosenbaum and Kariya 1987; Reubens 1977).

This helps explain why, in math and science, American students compare unfavorably to their peers overseas (International Association for the Evaluation of Educational Achievement 1988; McKnight et al 1987), and why so many observers of American secondary education have remarked on how little energy students seem to devote to learning. John Goodlad (1984) observed, "The extraordinary degree of student passivity stands out." Theodore Sizer (1984) concluded, "No more important finding has emerged from the inquiries of our study than the American high school student, as student, is all too often docile, compliant and without initiative." One cause of this phenomenon may be the failure of the economy to give academic achievement its due reward in the labor market

and reward instead credentials that signify time spent rather than competencies obtained.

Footnotes

1. Test scores appear to have larger effects on the wages of those who go to college. Willis and Rosen found that for this population a one standard deviation increase in math and reading test scores raised wages by 3.6 percent in the first job after school and by 8.3 percent 20 years later. One of the reasons for academic achievement's greater effect on this population is the signals of academic achievement provided by the reputation of the college one attends.

2. For tests given to high school seniors a one standard deviation academic achievement differential is equal to 3.5 to 4 grade level equivalents ((GLE). We assume that range restriction reduced the variance of the test by a factor of 3 and that test retest reliability is .85. Then, $\partial Y/\partial GLE = .035(3)/.85(3.5) = .02$.

3. Ironically the court decision which sustained EEOC's power in this area (Griggs vs. Duke Power 401US424-1971) struck down the use of high school diplomas as a screening criterion for an entry level job. However, if schooling is not removed from the job application, there is no way of enforcing a ban against using schooling as a hiring criterion. Tests must be administered before they can be used, so EEOC has been much more successful in restricting their use. Griggs may also be part of the reason why employers do not insist that youthful job applicants bring high school transcripts when they apply for a job and have not complained more forcefully when local high schools do not respond to student requests that transcripts be sent to the employer.

4. If employers use pay to motivate workers and are not constrained by worker risk aversion, the optimal c_1 would be very close to 1. Since the V(e) is substantial this would imply that the variance of wages would have to exceed the variance of weighted averages of past productivity. Since relative productivity has been found to have only moderate effects on relative wages (Bishop 1987), it appears that worker risk aversion, union aversion to unconstrained merit pay or some other factor is resulting in employment contracts which recognize individual performance only in part.

5. The literature search began with two recent reviews of the industrial psychology literature on the subject (Schmidt and Hunter 1983 and Boudreau 1986). A number of other studies were tracked down through leads provided by John Hunter and by John Boudreau.

6. The job incumbents used to calculate these raw validity estimates have been through two different selection processes--first hiring and then retention--so these raw validity numbers are not estimates of population validities. For our purposes, however, raw validity estimates are what is required. They characterize how the conditional expectations of relative productivity vary with a worker's characteristics in a sample of job incumbents.

7. Theory suggests a number of factors which could cause $P_j - W_j$ to be non zero: adjustment costs, monopsony power, agency problems, and specific

human capital. If the firm were in disequilibrium due to a cyclical downturn, the size of the quasi rents would vary across jobs and their magnitude would probably be correlated with a worker's schooling or GIA. Specific human capital investments and monitoring costs are also both likely to be greater in the types of jobs that workers with high levels of schooling and GIA obtain. In all three cases, the time paths of productivity and wages that result have counter balancing periods of over and under compensation. Consequently, from a long run life cycle perspective, these quasi rents should net out to zero. Monopsony power and bargaining over the division of the firm's quasi rents, on the other hand, might generate non zero lifetime P_j-W_j's. Bishop (1978) examined the effect of queuing for union jobs on the social return to schooling and found that the lowered probability of taking union jobs that results from going to college raises the social return to college above the private return. The effect of queuing for union jobs on the social return to GIA was not investigated. An exploration of this and related issues is beyond the scope of this paper. It is an area that could benefit from more research.

8. Industrial psychologists generally refer to these tests as aptitude tests because from the employer's perspective they measure aptitudes that contribute to job performance. The paper refers to them as achievement tests because it takes the perspective of the educational system. While there is some controversy about how large schooling's effect is, there is no controversy about the proposition that additional schooling does improve test scores on all types of tests including those referred to as IQ and Aptitude tests (Lorge 1945; Husen 1951; Department of Labor 1970). Further evidence of the great sensitivity of IQ and other "aptitude" tests to environment comes from the fact that the IQ of young adults has been rising rapidly in Europe and Japan and until recently was rising rapidly in the US as well (Bishop 1987b; Flynn 1987).

9. The formula was $SD(R^m_j) = \sqrt{(R^m_{ij}-R^m_j)^2/N-1}$. Occasionally employers who had only 2 or 3 employees gave them all the same rating. Consequently, a lower bound of 40 percent of the mean $SD(R^m_j)$ was placed on the value the SD could take. Models were also estimated which did not standardize job performance variance across firms and which instead standardized the variances only across the occupation. None of the substantive findings were changed by this alternative methodology.

10. An alternative strategy would have been to attempt an estimate of the true productivity relationship (equation 1) by applying a fixed effects methodology to the GATB revalidation data. Estimates of the bias in wage functions might then be made by comparing the fixed effect results to a wage equation. This approach was rejected for 4 reasons: (a) it is hostage to the accuracy of our estimates of SD\$ and $SD(P^t_j)$, (b) the crucial hypothesis tests necessitate a comparison of parameters estimated in very disparate data sets, (c) it depends on an assumption--the average quality of a workforce has the same effect on average productivity as deviations of worker quality from the average have on deviations of productivity from the average--which is almost certainly wrong, and (d) parameters of the estimated model will probably be biased

by the selectivity of hiring, turnover and promotions.(Brown 1978; Mueser and Maloney 1987)

11. The variance of the measurement error of schooling, .94, was at the lower end of the range reported by Bishop (1974) and Jencks et. al (1979). G and N were assumed to be indicators of the unobservable GIA. With G and N each assigned a POPSD of 1.0, the variance of their measurement error was .125 and .155 respectively. Since the arithmetic operations test was a component of both G and N, the covariance of v_2 and v'_2 was assumed to be .06125. Gender, Black and Hispanic were assumed to be measured without error. Reliability was assumed to be .9 for age, age squared, occupational experience and occupational experience squared and .95 for tenure and tenure squared. The correlation between the measurement errors of a variable and its square was assumed to be equal to the correlation between the underlying variable. The estimated coefficient on schooling and GIA were not sensitive to estimation technique (OLS or maximum likelihood) or to assumptions regarding measurement error in the control variables (gender, black, Hispanic, age, tenure and occupational experience). The measurement assumptions that make a difference are those that relate to the reliability of schooling, GIA and the job performance measure.

12. The industrial psychology literature contains a good deal of discussion of the proper method for measuring SD$ and SD(P^L_J), but most authors fail to discuss the possibility of a discrepancy between the two and there does not appear to be any studies which demonstrate the two to be equivalent (Boudreau 1986, Schmidt and Hunter 1983). It would appear to this author that SD(P^L_J) may be less than SD$ and if so the estimates of the percentage and dollar effects of GIA on relative productivity that appear in the conclusion may have to be reduced. More research is needed on this issue.

13. The estimate of SD$ for specific occupations were taken from Table 1. The ratio of SD$ to compensation of service workers was assumed to be the same as for clerical and semi skilled workers. The ratio of the earnings of the jobs in the GATB data to the earnings of all jobs was calculated from Table 281 of the 1980 Census. The weights were .08 for technician, .224 for clerical workers, .027 for plant operators, .324 for craft workers, .281 for semi skilled blue collar workers and .065 for other service workers. The National Income Accounts provided the $25,289 estimate of average compensation per full time equivalent employee.

14. The fact that very few firms had to be excluded from the study because they were using tests during the 1972 - 1982 period indicates that most employers had no access to the GIA scores of their employees. Employers might, however, have other less formal mechanisms of assessing GIA and might base pay decisions on these assessments. This hypothesis cannot be tested here because none of the data sets available contain measures of both GIA and within-job relative wages. Research into this issue is needed. However, the effect of GIA on within-occupation relative wage rates was studied and results for blue collar and clerical occupations are presented below.

15. Medoff and Abraham report regressions in which salary is predicted by education, experience, grade level dummies and dummies for performance rating (Table 1 and II 1980a and Table 1 1980b). It was assumed that the underlying distribution of job performance was normal. The mean Z scores were calculated for the top rating category (Z_t) and for the two lowest rating categories combined (Z_1). The wage effect of a one SD performance differential was calculated by the formula $(b_t - b_1)/(Z_t - Z_1)$ where b_1 is a weighted average of the coefficients on the two low rating categories. Rough adjustments for measurement error were made by dividing these estimates by .6, the reliability of the rating scale.

16. Gerhart and Milkovich (1987) enter the performance rating linearly into log salary level and log salary growth regressions. The standard deviation of this variable is .56 in 1980 and .54 in 1986. The coefficient on the 6 year average rating (which has an SD of .37) was .1035. A one SD (.55) increase in rating for one year raises ones lifetime salary level by .00949. [.1035 x .55 /6]. A rough correction for measurement error was made by dividing .00949 by .6, the reliability of the rating scale. Assuming an infinite lifetime and a discounting factor of 10 percent, the present discounted value of a permanent 1.55 percent wage increase is 15.5 percent of compensation for one year.

17. For example, alternative form reliabilities average .75 for 7 SRA subtests and .87 for the G aptitude of the GATB. The G aptitude of the GATB has an average correlation of .7 with the 7 SRA subtests, .75 with the WAIS verbal IQ, .78 with the ITED composite and .81 with the ACT composite (Hunter, Crosson & Friedman 1985; Department of Labor 1970). There are good reasons for high correlations between past achievement and scores on "aptitude" tests designed to predict future achievement. Past achievement aids learning because the tools (e.g. reading and mathematics) and concepts taught early in the curriculum are often essential for learning the material that comes later. Furthermore "aptitude" tests are validated on later achievement levels not on rates of change of achievement. Consequently, the items that tend to be included look a lot like the items that appear on achievement tests.

19. One potential challenge to this conclusion comes from the possibility that these discrepancies reflect a tendency to reward academic achievement in invisible ways such as praise, perks and higher social status rather than through more visible mechanisms such as wage increases and promotions(Frank 1984). If these rewards were large and anticipated by students when deciding about the effort to apply to their studies, there might be no tendency for students to underinvest in learning. This might be part of the story but it cannot be the whole story. The reason for this conclusion is that workers are risk averse and relative productivity cannot be measured with perfect reliability. These two facts will result in under compensation of real improvements in productivity even if the compensation comes in a form that is invisible to the analyst.

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Table 1

GENERAL INTELLECTUAL ACHIEVEMENT AND PRODUCTIVITY ON THE JOB

	Raw Validity of GIA		Coefficient of Variation	Standard Deviation of Output[2] in 1985$	Percent of NonFarm Business in Occupation
	Ghiselli	Recent Estim.[1]			
Professional	--	.43	--	--	7.5
Technical	--	.32	.36	$ 16,210	3.1
Executive	.30	--	--	--	0.5
Administrative	.30	.35	.33	$ 9,501	9.9
Sales (Exc. Retail & Personal Service)	.34	.27	.50	$ 13,660	5.8
Sales Clerk (Retail & Personal Service)	-.06	.14	.38	$ 6,130	6.3
Clerical	.27	.26	.21	$ 4,940	16.8
Foremen	.28	--	--	--	3.4
Plant Operators	--	.18	.54	$ 74,642	.3
Other Craft Occupations	.25	--	.27	$ 12,383	11.6
Semi Skilled and Unskilled Factory	.20	--	.20	$ 6,392	16.5
Transportation Equipment Operatives	.16	--	--	--	5.1
Protective Occupations	.23	.27	--	--	0.0
Other Service	.26	.27	--	--	13.4
					100.0

[1]The raw validity estimates for professional, technical, administrative protective occupations and other service workers are averages of studies reported in the GATB manual. The estimate for clerical workers is from Pearlman, Schmidt and Hunter (1980). The estimate for sales except retail and service is based on Churchill et al's (1985) examination of 44 studies using objective company data with controls for environmental conditions. The estimate for plant operators is an average of results from Dunette et al (1984, Table 5.38) and from Schmidt, Hunter and Caplan, 1983, Table 4.

[2]The estimates of SD of output are from the review of the 34 studies presented in tables 1, 2 and 3.

Table 2
Effects of Academic Achievement on Relative Job Performance
All Workers
Equation 8

	General Intellectual Achievement	Mathematical Achievement	Years of Schooling	GIA College Interact.	GIA Tenure Interact.	GIA Size Interact.	Controls for Race Gender	Controls for Age, Tenure & Exper.	R^2
1.	.073 (6.38)	.098 (8.99)	—	—	—	—	—	—	.028
2.	.039 (3.26)	.095 (8.65)	—	—	—	—	✓	—	.035
3.	.064 (5.35)	.103 (9.55)	-.024 (7.12)	—	—	—	✓	✓	.073
4.	.045 (3.49)	.107 (9.90)	-.026 (7.54)	.053 (4.16)	—	—	✓	✓	.073
5.	.053 (3.92)	.108 (9.95)	-.025 (7.49)	.050 (3.87)	-.024 (1.88)	—	✓	✓	.074
6.	.050 (3.75)	.108 (9.93)	-.026 (7.58)	.052 (4.06)	-.023 (1.91)	.007 (5.94)	✓	✓	.074
7.	.321 (21.57)		-.079 (9.41)	—	—	—	✓	✓	.137

Source: Analysis of 31399 observations from US Employment Service Individual Observation Data File. The metric of GIA and Mathematical Achievement is a POP SD. The metric of Job Performance is within firm standard deviation of the performance rating scale. Productivity relative to coworkers is modeled as a function of background characteristics. Models 1-6 do not correct for errors in measurement and are thus estimates of equation 8. Model 7 is an estimate of the measurement model, the system of equations represented by (8) and (9). The estimated value of λ was 1.029 with a standard error of .003.

Table 3
Equation 8

Effects of Academic Achievement on Job Performance
by Occupation

Occupational Category	General Intellectual Achievement	Mathematical Achievement	Years of Schooling	R^2	N	GIA (Pop. Mean=0)	Effect of a POP SD of GIA & M w/o controls
Technicians	.101 (2.43)	.098 (2.45)	.004 (.29)	.076	2384	.081	.299
Clerical	.062 (2.50)	.140 (6.06)	-.009 (1.03)	.088	6694	-.025	.346
High Skill Blue Collar	.072 (3.46)	.089 (4.68)	-.021 (3.57)	.081	10477	-.088	.272
Low Skill Blue Collar	.076 (3.10)	.099 (4.73)	-.032 (4.74)	.085	8402	-.693	.227
Service	.154 (3.04)	.139 (3.04)	-.028 (2.11)	.100	1927	-.632	.302

The metric of GIA and Math Achievement is a population SD.
The metric of Job Performance is the within firm standard deviation of the performance rating scale.
Productivity relative to coworkers is modeled as a function of background characteristics.
Controls were included for gender, black, hispanic, age, tenure and total occupational experience and their squares.
Errors in measurement of job performance, GIA and Years of Schooling have not been adjusted for.

Table 4

Effects of Academic Achievement on Job Performance
Administrative Jobs in the Federal Government

	GIA	Years of Schooling	Tenure	R^2	Num. of Obs.
Internal Revenue Service Officer	.865	.049	-.001	.343	285
Work sample	(7.2)	(.9)	(.1)		
Supervisory Ratings	.400	-.043	-.054	.112	285
	(2.7)	(.6)	(3.5)		
Social Insurance Claims Adjuster	.552	.016	-.012	.148	218
Work sample	(3.7)	(.4)	(.5)		
Supervisory Ratings	.520	-.006	-.044	.158	218
	(3.2)	(.1)	(1.5)		
Customs Inspector	1.032	-.119	-.023	.369	185
Work sample	(6.9)	(1.8)	(1.6)		
Supervisory Ratings	.266	-.013	.061	.121	185
	(1.0)	(.1)	(2.5)		

Source: Analysis of data from 3 studies of the validity of the written portion of the PACE.

The metric for GIA is population standard deviations and the range restriction factor is .666. The reliability of the GIA test is .93. The reliabilities of the three work samples were .78, .72 and .80 respectively. The reliabilities of the supervisory ratings were .60, .60 and .34 (Hunter 1983). The variance of measurement error was .554 for schooling and 2.987 for tenure. This translates into reliabilities for the restricted sample of .80 for schooling and .912 for tenure (Jencks et al 1979).

APPENDIX AVAILABLE FROM AUTHOR

ON STUDIES OF OUTPUT VARIABILITY

A search for studies of output variability yielded 27 published and 7 unpublished papers covering 69 distinct jobs. Their results are reported in tables 1, 2 and 3. Table 1 summarizes the studies of output variability among semiskilled factory workers. In these studies it was possible to measure physical units of output. For the jobs that were paid on an hourly basis the ratio of the standard deviation of output to mean output, coefficient of variation or CV, averaged about 20 percent. The jobs paid on a piece rate basis typically had smaller coefficients of variation (15 percent on average). Apparently when workers are paid on a piece rate basis, quit rates are much more responsive to productivity than when pay is on an hourly basis. The less productive workers self select themselves out of such jobs and the surviving job incumbents become more and more similar in their output.

When a firm expands by hiring extra workers, it incurs significant fixed costs. It must rent space, buy equipment, hire supervisors and recruit, hire, train, and payroll the additional production workers. If output can be increased by hiring more competent workers, all of these costs can be avoided and the firm's capital becomes more productive. These factors tend to magnify the effects of work force quality on productivity. They imply that the ratio of the standard deviation of worker productivity in dollars (SD$) to average worker compensation is much larger than the productivity CV for that job (Klein, Spady and Weiss 1983; Frank 1984).

Estimates of productivity standard deviations (SD$) in 1985 dollars are reported in column 3 of the table. In most cases the author of the study made no attempt to estimate SD$'s, so the estimate has been calculated from the CV. Such estimates are placed in a parenthesis. The estimates of SD$ were derived as a product of the CV, the mean compensation for that job and the ratio of value added to compensation for that industry (for manufacturing as a whole this ratio is 1.63). The value added to compensation ratio in retailing and in real estate was much too high to be used as an adjustment factor. So for all sales occupations it was assumed that SD$ = CV times average compensation. The SD$ of semiskilled factory jobs ranged from $2329 to $16775 and averaged $6392 for jobs not known to be paid on a piece rate.

What about jobs where capital equipment controls the pace of work?

It has been argued that in automated continuous process industries the amount and quality of output is determined by technology and computer programs not by the skills and talents of the workers. In fact, however, programs cannot be written to handle all contingencies and machines are never completely reliable so human operators have an important role to play (Hirschorn 1984; Adler 1986). In capital intensive industries with high rates of energy and materials consumption, small errors can cause substantial losses. Small adjustments which increase fuel efficiency can save a utility or refinery millions of dollars a week. This has been demonstrated by two very thorough studies of the variability of the job performance of plant operators (see Table 2). In a study of the operators of electric generating plants commissioned by the Edison Electric Institute, committees of technical experts were organized and asked to make consensus estimates of the frequency and costs of the most common types of operator errors. Once the relationship between specific operator errors and the purchase costs of replacement power was established, the experts estimated what would be expected (in dollar terms) from an operator at the 15th, 50th and 85th percentile of job performance. The study concluded that the standard deviation for the productivity of control room operators is about $278,000 in 1985 dollars at nuclear plants and $115,000 at fossil fuel plants (Dunnette et al 1982).[1] The standard deviation of productivity for operators of refineries was also large - ranging from $15399 for the head operators to $10381 for the pump operators (Wroten, 1984). Table 2 also reports managerial estimates of coefficients of variation and productivity SD$'s for a number of craft occupations. The average CV is 27 percent and the average SD$ is $12,383. These are smaller than for plant operators and larger than those for semi-skilled factory workers.

Output variability is also great in professional and high level managerial occupations. Users of communication satellites, for example, are going to save billions of dollars as a result of a discovery by a scientist at Comsat which has doubled the effective lifetime of satellites. Exxon had invested a billion dollars in its shale oil operation at Parachute Creek before giving up on the enterprise. A wiser CEO or better staff work might have avoided or reduced this loss. It does not take many such examples to produce a very large standard deviation of output for professional and high level managerial jobs. In most white collar jobs, however, output variability across incumbents is much smaller.

Table 3 reports the results of studies of output variability in these occupations. In many of these studies hard measures of output (e.g., sales, cards punched) were the basis for calculating coefficients of variation. For a number of occupations physical measures of output were not definable so the supervisors were asked to report dollar amounts of output expected from workers at the 15th, 50th and 85th percentiles of the job performance distribution. For clerical jobs the CV averaged 20.5 percent and the SD$'s averaged $4940. For sales clerks the CV averaged 38 percent and the SD$'s $6130. Coefficients of variation averaged 36 percent for technicians, and 33 percent for managers. For higher level sales personnel, many of whom were paid on a commission basis, the coefficient of variation was 50 percent. Coefficients of variation appear to be higher in jobs with greater cognitive complexity and in sales jobs. While the SD$'s are substantial even in low wage clerical jobs there is clearly a positive correlation between average wage levels and SD$'s. Except for the higher level sales personnel and the convenience store managers these white collar jobs are not paid commissions or bonuses.

While it is possible to debate the accuracy of specific estimates and the reliability of the 15th, 50th, and 85th percentile method of measuring SD$, one would have to take the extreme view that SD$ is almost zero before the basic conclusion that workers paid the same wage are often significantly different in productivity would change. This implies that the $W_i=P_i$ assumption cannot possibly be true.

1. Large as it may seem the estimate for operators of nuclear plants is in fact quite reasonable. In the first 4000 years of world wide operation of nuclear plants there have been two catastrophic accidents caused by operator error each costing over 5 billion dollars. The NRC estimates that improved safety procedures will reduce operator caused catastrophic accidents to about one fifth that rate (one in every 10000 years of plant operation). There are 5 six person shifts operating each plant, so the standard deviation of output across individual workers that results from just this one risk is about $9 million per year.

Methods used to Estimate the Coefficient of Variation and Standard Deviations of Output

PO - Physical Output - Where a piece rate prevails, ticket earnings are used as the output measure. Where pay is hourly, physical quantity of output or percent of standard output for the job is used as the output measure. CV's are calculated from this data and SD$s are constructed by using value added per employee (adjusted for relative wage rates) to value the productivity of the average worker.

WS - Work Sample - A sample of the job tasks is taken and workers are observed performing these tasks under controlled conditions. To be useful for calculating a CV, the WS must be defined in units that have a ratio scale that corresponds to output such as 50 lb sacks carried from A to B. It measures peak performance and thus probably does not measure effort as actually applied to a real job. SD$s are calculated from CV's in same way they are calculated from PO based CV's.

GS - Gross Sales - CV's are the SD of sales across sales personnel divided by the mean level of sales. SD$ equals the CV times the mean compensation of sales personnel. GS(A) is calculated using a weighted average of the sales of different products.

SHMM - Schmidt, Hunter, McKenzie and Muldrow (1979) Method. Managers who supervise job incumbents are asked to place monetary values on the output produced by an employee at the 15th, 50th and 85th percentile of the job performance distribution. The metric in which they are asked to make these judgement is the cost to have an "outside firms provide these products and services." This yields direct estimates of SD$ and a rough estimate of the CV can be calculated from $(P_{85} - P_{15})/2P_{50}$.

S(m) - Schmidt et al (1979) method with supervisors making their judgments after being supplied a mean output derived from company records.

S(T) - Schmidt et al (1979) method with outliers dropped from the calculation.

SE - Supervisor's Estimate for actual employees. Supervisors give dollar values for the productivity of a sample of actual employees. The mean and standard deviation is calculated from this distribution.

S(D) - Schmidt et al (1979) method as modified by Dunnette et al (1982). A first round of workshops with supervisors identified examples of unusually effective, unusually ineffective and average levels of job performance by operators. Eight dimensions of performance were developed from these examples and supervisors were asked to retranslate and scale the 667 performance examples in a second round of workshops. Finally participants were asked to estimate dollar value of performance at the 85th, 50th and 15th percentile. Negative values were changed to zero.

TABLE 1

UNSKILLED AND SEMISKILLED BLUE COLLAR WORKERS

		Method	CV of Output (Incumbents)	Standard Deviation in 1985 Dollars[a]	Raw Validity of GMA[b]
Hourly Pay					
Assembly Worker	(2b)	PO	14	($4413)	20
Radial Drill Operator	(20)	PO	25	$7881	20
Butter Wrappers	(21a)	PO	23	($5206)	20
Machine Operators	(21b)	PO	25	($7881)	20
Coil Winders	(21f)	PO	19	($4892)	34
Machine Operators	(21g)	PO	14	($4413)	20
Welders	(21d)	PO	19	($6988)	20
Welders-Refinery	(29)	SE	12	$16775	21
Electrical Workers	(25)	PO	19	($4892)	20
Entry Level Steelworkers	(1)	WS	19	($8177)	13
			19.7	$7152	
Piece Rate					
Lathe Operator	(2a)	PO	15	($4728)	20
Assembly Workers	(2b)	PO	11	($3467)	20
Chocolate Dippers	(21c)	PO	12	($2716)	20
Foundry Workers	(21e)	PO	12	($3783)	20
Machine Operators	(21g)	PO	14	($4413)	20
Electrical Workers	(25)	PO	21	($5407)	20
Hosiery Looper	(25)	PO	19	($2881)	20
Weavers	(26)	PO	14	($2123)	23
			14.75	$3690	
Pay Form: Unknown					
"Handcraft Workers"	(9)	PO	23	($7250)	20
Drillers	(12)	PO	33	($10402)	20
Wool Pullers	(12)	PO	20	($3033)	20
Cable Makers	(15)	PO	24	($6180)	29
Electrical Workers	(15)	PO	19	($4892)	20
Assemblers	(15)	PO	26	($8196)	20
Machine Sewers	(28)	PO	20	($2329)	20
Electrical Workers	(28)	PO	15	($3862)	20
Lamp Shade Manufacturer	(24)	PO	12	($3783)	20
			21.3	$5547	

Footnotes for Table 1

"Estimates of standard deviation of the output (SD$) of full time full year workers that are presented in parenthesis were derived from coefficients of variation (CV) for output. For jobs outside of mining, retailing and finance it was assumed that a more capable worker would necessitate proportionately more materials, energy inputs, overhead labor inputs but not necessitate additional capital. This means that the metric of the CV is K-L productivity and thus that in manufacturing where the ratio of value added to compensation is 1.4, a 10 percent gain in K-L productivity has a dollar value equal to about 14 percent of compensation. Consequently, $SD\$_j = CV_j$ (GNP per full time equivalent worker in industry k)$(wage_{kj}/(wage_k)$ where $wage_{kj}$ = average wage of occupation j in industry k and $wage_k$ is average wage in industry k. The ratio of occupation "j"s earnings to the industry average was derived from Table 2 of Occupation by Industry Subject Report of the 1980 Census.

"Where job specific validity estimates were not available a raw validity estimate of .2 was taken from Ghiselli (1973). It is based on over 10,000 cases. The raw validity estimates for welder-refinery is from Schmidt, Hunter, Caplan (1981) and is based on 821 cases. The value for entry level steelworkers (.13) is the value for the lowest complexity category of jobs and is from Hunter (1983). The other estimates are from the GATB manual and are based on 65 coil winders, 100 cable makers and 94 weavers.

Table 2

PRECISION PRODUCTION AND CRAFT OCCUPATIONS

	Method	C.V. of Output (Incumb)	Standard Deviation in 1985 Dollars	Raw Validity of GMA
Plant and System Operators				
Nuclear Control Room Oper. (8)	S(D)	108	$277,850	.10[a]
Fossil Fuel Control Room Oper. (8)	S(D)	72	$155,340	.26[a]
Nuclear Plant Operator (8)	S(D)	105	$ 97,370	.10[a]
Fossil Fuel Plant Operator (8)	S(D)	61	$ 39,455	.26[a]
Hydro Plant Operator (8)	S(D)	53	$ 27,030	.11[a]
Refinery Head Operator (29)	SE	--	$ 15,355	.21[b]
Outside Operator (29)	SE	--	$ 14,356	.21[b]
Pump Operator (29)	SE	--	$ 10,381	.21[b]
			$ 74,642	
Other Craft Workers				
Outside Mechanic (29)	SE	--	$ 21,800	.18[b]
Electrician (14)	SHMM	23	$ 12,539	.25[c]
Sheet Metal Worker (14)	SHMM	25	$ 11,696	.25[c]
Plumber (14)	SHMM	24	$ 11,856	.25[c]
Painter (14)	SHMM	24	$ 8,626	.25[c]
Meat Cutter (14)	SHMM	26	$ 7,778	.25[c]
Maintenance & Tool Room Jobs (4)	PO	40	--	.25[c]
			$ 12,383	
Supervisors				
Steel: Foreman (average) (18)	SHMM	--	$ 67,923	.28[d]

[a]Raw validity from Dunette et al Table 5.38 based on 460 nuclear operators, 1,388 fossil operators and 186 hydro & switchboard operators. Criterion reliability was .71 for fossil fuel operators and .56 for nuclear & hydro operators. The data was collected in 1981 so the inflation factor based on the growth of utility wages and salaries per FTE is 1.30.

[b]Raw validity from Schmidt, Hunter & Caplan (1981), Table 4 based on 1,486 operators and 821 maintenance workers. Criterion reliability = .7. The inflaction factor since 1983 is 1.10 for petroleum refinery industry.

[c]Raw validity from Ghiselli (1973) based on over 10,000 workers. Criterion reliability = .6.

[d]Raw validity from Ghiselli (1973) based on between 5,000 and 10,000 workers. Criterion reliability = .6. The inflation factor for the steel industry study is 1.084 for 1985 vs. 1982.

TABLE 3

WHITE COLLAR WORKERS

		Method	CV of Output (Incumbents)	Standard Deviation in 1985 Dollars	Raw Validity of GMA
Technical					
Budget Analyst	(10)	SHMM	(47)	$15062	.32[b]
Park Ranger	(23)	SHMM	33	$ 4828	.28
Computer Programmer	(22)	SHMM	32	$16550	.38[a]
Computer Programmer	(18)	SHMM	47	$15888	.38[a]
Instrument Technician-Refinery	(29)	SE	(20)	$28720	.21
			36	$16210	
Managerial					
Convenience Store Managers	(27)	SHMM	51	$13967	.35[c]
Bank Branch Manager	(16)	S(T)	(35)	$10064	.35[c]
Bank Operations Manager	(16)	S(T)	(14)	$ 3122	.35[c]
			33	$ 9051	
High Level Sales					
District Sales Rep.Mfg	(5)	GS	43	($18590)	.27[d]
Insurance Salesman	(3)	GS(A)	42	$5831[f]	.27[d]
District Sales - Food Manufacturing	(7)	GS	32	($8958)[a]	.27[d]
Real Estate Sales	(14)	SHMM	83	$21271	.27[d]
			50	$13660	
Clerical Workers					
Claims Processors	(13)	PO	31	$5542	.26[g]
Clerical	(6)	S(M)	25	$5529	.26[g]
Ticket Agent	(14)	SHMM	26	$8411	.26[g]
Meter Reader	(14)	SHMM	18	$4481	.26[g]
Card Punch Operator	(11)	PO	12	($2539)	.26[g]
Proof Machine Operator	(11)	PO	14	($2962)	.26[g]
Typists	(24)	PO	19	($4020)	.26[g]
Card Punch Operator	(24)	PO	16	($3385)	.26[g]
Card Punch Operator	(24)	PO	29	($6135)	.26[g]
Head Teller-Bank	(16)	S(T)	(15)	$2369	.26[g]
			20.5	$4940	
Sales Clerk					
Cashier Checker	(14)	SHMM	43	$11379	.04[b]
Cashiers	(12)	GS	20	($2433)	.04[b]
Sales Clerks	(12)	GS	54	($6570)	.04[b]
Sales Clerks	(24)	GS	34	($4236)	.04[b]
			38	$6130	

Footnotes for Table 3

[a]The Programmer Aptitude Tests raw validity is .38 based on Schmidt, Rosenberg and Hunter's (1980) validity generalization of data on 1299 programmers.

[b]The estimate of GMA job performance raw validities for technical jobs is based on 20 occupations and a total of 2417 cases. The estimate for professional occupations is based on 2 occupations and a total of 109 cases. Schmidt, Mack & Hunter classify the park ranger job as a level 3 job using Hunters (1983) classification scheme. For a level 3 job the raw validity of GMA is .28.

[c]GMA raw validity for managers is a simple average of 9 seperate managerial occupations from the GATB manual.

[d]The raw validity estimate is from Churchill et al's "The Determinants of Sales Person Performance: A Meta-Analysis" (1985) and is based on 44 studies which used objective company data with controls for environmental conditions. Since actual sales data were used it is assumed that criterion reliability is 1.0.

[e]Cascio and Silbey estimated the average compensation of sales personnel to be $75 a day or $18000 a year in 1978. This was inflated to 1985 wage levels by multiplying by 1.555 and then multiplied by CV to estimate SD$.

[f]Bobko et al, SHMM type estimate of SD$ was $4967 which is inflated to 1985 wage levels by multiplying by 1.174 the growth of wages and salaries in the industry from 1982 to 1985.

[g]Pearlman, Schmidt, and Hunter 1980.

[h]Validity estimate for sales clerk jobs is an average of Ghiselli's estimate (-.06) and the mean of more recent studies (.14) is reported by Hunter and Hunter (1984).

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NAME OF WORKER (Print) (Last) (First)

SEX: MALE_____ FEMALE_____

Company Job Title: _____

How often do you see this worker in a work situation?

- ☐ All the time.
- ☐ Several times a day.
- ☐ Several times a week.
- ☐ Seldom.

How long have you worked with this worker?

- ☐ Under one month.
- ☐ One to two months.
- ☐ Three to five months.
- ☐ Six months or more.

A. How much can this worker get done? (Worker's ability to make efficient use of time and to work at high speed.) (If it is possible to rate only the quantity of work which a person can do on this job as adequate or inadequate, use #2 to indicate "inadequate" and #4 to indicate "adequate.")

- ☐ 1. Capable of very low work output. Can perform only at an unsatisfactory pace.
- ☐ 2. Capable of low work output. Can perform at a slow pace.
- ☐ 3. Capable of fair work output. Can perform at an acceptable pace.
- ☐ 4. Capable of high work output. Can perform at a fast pace.
- ☐ 5. Capable of very high work output. Can perform at an unusually fast pace.

B. How good is the quality of work? (Worker's ability to do high-grade work which meets quality standards.)

- ☐ 1. Performance is inferior and almost never meets minimum quality standards.
- ☐ 2. Performance is usually acceptable but somewhat inferior in quality.
- ☐ 3. Performance is acceptable but usually not superior in quality.
- ☐ 4. Performance is usually superior in quality.
- ☐ 5. Performance is almost always of the highest quality.

C. How accurate is the work? (Worker's ability to avoid making mistakes.)

- ☐ 1. Makes very many mistakes. Work needs constant checking.
- ☐ 2. Makes frequent mistakes. Work needs more checking than is desirable.
- ☐ 3. Makes mistakes occasionally. Work needs only normal checking.
- ☐ 4. Makes few mistakes. Work seldom needs checking.
- ☐ 5. Rarely makes a mistake. Work almost never needs checking.

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D. How much does the worker know about the job? (Worker's understanding of the principles, equipment, materials and methods that have to do directly or indirectly with the work.)

- ☐ 1. Has very limited knowledge. Does not know enough to do the job adequately.
- ☐ 2. Has little knowledge. Knows enough to get by.
- ☐ 3. Has moderate amount of knowledge. Knows enough to do fair work.
- ☐ 4. Has broad knowledge. Knows enough to do good work.
- ☐ 5. Has complete knowledge. Knows the job thoroughly.

E. How large a variety of job duties can the worker perform efficiently? (Worker's ability to handle several different operations.)

- ☐ 1. Cannot perform different operations adequately.
- ☐ 2. Can perform a limited number of different operations efficiently.
- ☐ 3. Can perform several different operations with reasonable efficiency.
- ☐ 4. Can perform many different operations efficiently.
- ☐ 5. Can perform an unusually large variety of different operations efficiently.

F. Considering all the factors already rated, and only these factors, how good is this worker? (Worker's all-around ability to do the job.)

- ☐ 1. Performance usually not acceptable.
- ☐ 2. Performance somewhat inferior.
- ☐ 3. A fairly proficient worker.
- ☐ 4. Performance usually superior.
- ☐ 5. An unusually competent worker.

Complete the following ONLY if the worker is no longer on the job.

G. What do you think is the reason this person left the job? (It is not necessary to show the official reason if you feel that there is another reason, as this form will not be shown to anybody in the company.)

- ☐ 1. Fired because of inability to do the job.
- ☐ 2. Quit, and I feel that it was because of difficulty doing the job.
- ☐ 3. Fired or laid off for reasons other than ability to do the job (i.e., absenteeism, reduction in force).
- ☐ 4. Quit, and I feel the reason for quitting was not related to ability to do the job.
- ☐ 5. Quit or was promoted or reassigned because the worker had learned the job well and wanted to advance.

RATED BY	TITLE	DATE
COMPANY OR ORGANIZATION	LOCATION (City, State, ZIP Code)	

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